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CHARLES J. HITCH

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November 17, 1960

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NOTE

This paper presents the text of an address given by Mr. Hitch at the ceremonies dedicating the Center for Advanced Study of The Brookings Institution, Washington, D.C., on November 17, 1960.

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THE USES OF ECONOMICS¹

There have been times, places, and audiences in the history of economics that would have demanded defense of so blatantly normative a title as the one I have been assigned for this address. I am simply going to assume that this audience does not. The greatest names in the history of our discipline were unashamedly normative—Adam Smith, Ricardo, Marshall, Keynes. This has never troubled me, as it has troubled many of my contemporaries. On the contrary, I chose economics as my profession because my first taste of it in college raised the thrilling hope that it could point the way toward solutions of some of the great public policy problems of our age. I still feel that if it cannot, it is a poor relation among the sciences, with a subject matter intrinsically less interesting and challenging than that of physics, chemistry, biology, psychology, or sociology. While these are personal feelings, they are far from unique. I know they are widely shared by the economists associated with Brookings, which has done so much research on public policy, and which has so many capabilities and such special opportunities for bringing the tools of our craft to bear upon it.

¹I am indebted to my colleagues Malcolm Hoag and Roland McKean for helpful suggestions and criticisms.

So I will take it for granted that economics should be useful rather than try to persuade you that it should be. If it is useless, it is unimportant. I shall begin by asking: *How* can economics be useful? Then I shall attempt to probe in greater detail the question: For *what problems* can it be useful? Finally, I shall twist my title a little and enquire: How useful are *economists*, and what can we do to make them more so?

How can economics be useful? First, and fundamentally, by providing an incisive and productive way of looking at problems.

Economics began as a study of the *economy*—of those activities associated with the production, exchange, and distribution of wealth, and especially of those related to markets and money. But because it studied these activities with normative objectives, it developed a theory of *economizing*, or of efficiency, which provided a criterion for judging the institutions of the economy, and a framework for improving those institutions. So important was this development in economic theory that many prominent economists have attempted to define economics in terms of economizing, or efficiency, or the logic of choice. I am not going to get involved today in questions of definition, but I am concerned to stress that economic theory and techniques have application and utility outside and beyond what we normally regard as the market economy.

Our magic way of looking at problems, or of economizing, is deceptively simple. Stripped to its essentials, it consists in arraying alternatives, estimating the utilities and costs of each, and choosing the alternative that yields the greatest excess of

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utilities over costs. To a well-trained economist, this procedure seems so natural and obvious—so common sensical—that he is likely to dismiss it as trivial. One of the important things I have learned in twenty years of intimate contact with noneconomists of all kinds—civil servants, engineers, scientists, and politicians—is that it is not an obvious procedure to other people, and is therefore far from trivial. The mere casting of a problem as an economic problem, the explicit arraying of the alternatives, and the qualitative evaluation of the costs and utilities of each, frequently throws a flood of light on the problem, revealing an answer, or a probable answer, or a partial answer, or at least pointing the way to the additional information needed to find an answer. When I went to RAND in 1948, I expected the principal role of the economists to be the supply of economic inputs to military and strategic problems, i.e., inputs from the economies involved, like weapon costs and the economic effects of strategic bombing. Instead, our principal contribution has turned out to be the far more critical one of helping to formulate the problems themselves—as economic problems—and to design analyses to solve them.

Let me further emphasize this point by reminding you of the widespread and (to us) incredible misconceptions of the nature of costs. An extreme (but common) view is: "What do costs matter when national security is at stake?" The only slightly more sophisticated view, beyond which people who are not economists seldom get, is that costs are simply a constraint, a test of feasibility. It is useful (but difficult) to demonstrate that in choosing among alternatives the costs are as essential as the

objectives—that they are both as integral to the process of choice as is implied by our phrase “opportunity costs.”

But of course very frequently the mere common-sense casting of a problem as an economic problem, while helpful for straight thinking, is far from sufficient for its solution. The second way in which economists can be useful, then, is in making the requisite economic analysis—usually a quantitative one—to improve choice among the alternatives. This will require empirical inputs only some of which would normally be regarded as economic. It will also require, as a rule, much more than the bare essentials of the logic of choice. And economists, with their well-developed corpus of theory, can go far beyond those bare essentials. It may appear necessary, for example, to find ways of making costs or objectives commensurable: we have developed a good deal of clever theory for that purpose, using market prices or shadow prices, as well as techniques—such as those employing the “efficient point” concept—for use when objectives and costs or two objectives cannot be expressed in terms of a common measure. Often the number of conceivable alternatives will be so large that most cannot be individually appraised: the economist in these circumstances may find good reason to conjecture that returns are not increasing over wide ranges, so that a few calculations of marginal changes from a single alternative will permit him legitimately to leap over hundreds of possible alternatives without tedious calculations for each. Just being sensitive to the possibilities of increasing returns helps the economist distinguish permissible from impermissible simplifications, as does his

preoccupation with marginal rather than average measurements. Similarly, his sophistication about utility will guard him against some misleading measures of performance, while assuring him that other partly arbitrary measures will be appropriate for specific purposes. He knows, at least in principle, how to deal with troublesome "spillover" effects, positive and negative, in sectors outside his analysis, and with risks and uncertainties. Economists have abundantly demonstrated their talent for the analysis of problems like these, where the variables are commonly regarded as economic, such as transportation and the development of water resources; they have also, to a more limited extent, been useful in precisely similar analyses, where the variables are not usually recognized as economic, like the military and strategic problems I have mentioned.

The third way in which economists seem to have a talent for being useful is in the design of institutional arrangements, broadly interpreted, which are conducive to economy or efficiency. This is the classic, traditional economist's prescription: fix the institutional environment in which economic activity takes place, and leave the rest to the invisible hand. We have become well aware during the past century and a half of the limitations of such prescriptions. But in stressing the limitations we have sometimes lost sight of the power and relevance of the concept. And that is a pity, because it is a tremendously useful concept stemming naturally from no other social science. There are powerful private incentives that can be harnessed to the public good if we can get the institutional setting right. Competitive markets do harness acquisitiveness. So do good property

laws. Freer trade does shift resources to more productive employment. A good patent system does stimulate invention. I can think of nothing that would contribute as much to the effectiveness of our defenses as some institutional arrangement that would give officials and commanders less incentive to fight for higher budgets and more to make the best use of what they have.

Next, let me address myself directly to the question: What problems can economics be useful in solving? I will confine myself today to the realm of public policy problems—not because I think economics is useless in solving personal and business problems, but because public policy is Brookings' business.

I should like to make a rough-and-ready distinction between the more traditional problems of economics in this area, and the newer and less traditional. I will say less about the traditional problems, because less needs to be said. I think that the record of economics in dealing with the traditional problems is one of which, on the whole, we can be proud. We have moved public policy in the direction of growth and freedom. We have made trade freer. We have stimulated competition. We have made progress in reducing fluctuations and unemployment.

But for this audience I need not stress the point that the traditional problems are far from solved. They will never be solved. Battles in political economy are never finally won, because social costs and benefits diverge, sometimes widely, from the private costs and benefits of many individual voters

and their representatives. And as we make progress on larger issues, smaller, subtler ones emerge to challenge our ingenuity.

There is, I think, a change in the general character of the important problems confronting us. Much of traditional economics has been broad and general. It has moved from broad principles to broad policies (like "free trade," "progressive taxation," "adequate aggregate demand"). Some of the newer areas—the economics of growth and development and the economics of government expenditure—do not seem to present such opportunities. They are full of messy empirical detail, involving technological and sociological inputs. The general principles are elusive, and not so general. The emerging policies are frequently little more than solutions to particular problems, limited in time and place. The residue of problems in the traditional public policy areas has this same general character. Some economists, in their frustration at such uncongenial chores, have denied that they constitute economics. And it is economics far removed from Ricardo.

Let me first run over the traditional public policy areas. The first and most classic is freer trade—the *non-agenda* of government. Despite our progress, restrictionism abounds in many guises and with many legitimate and illegitimate justifications. There is much good work remaining to be done. One of the most important tasks is the development of ways, means, and devices to make the removal of restrictions acceptable—of an *agenda* for government, if you like, to make possible the *non-agenda*. Without, for example, some combination of compensa-

tion, retraining, and subsidized mobility, I doubt that we can hold the line on tariffs, let alone make further progress. We may have to use more actual compensation, and place less reliance on the mere feasibility of compensation, to bring the private costs and benefits of voters into closer conformity with social costs and benefits.

Second, there is the traditional *agenda* of government, the area of monopoly-competition-regulation. This is an area deeply ploughed by Brookings in the past, and I am glad to see that it is still receiving the attention it deserves in Brookings' current program. My acquaintance with the very different position of monopolies and combinations in restraint of trade under English law has persuaded me that American policy on monopolies deserves more credit than it usually receives. I should like to see economists give increased attention to regulatory problems. To mention only a few examples, it seems to me that the policies and procedures of our regulatory commissions leave a great deal to be desired in the fields of transportation, communications, and utilities.

The economic aspects of some regulatory problems are scarcely recognized outside a few pages in our professional journals. Radio frequencies, for example, like land, water, and oil, have all the characteristics of an economic good; they are scarce and valuable, and rapidly becoming more so, and the efficiency with which they are used by the economy depends on the criteria used in allocating them. Yet we regard their allocation as a technological, administrative, and legal problem; we ration them without the slightest help from economic anal-

ysis or the market institutions that have proved invaluable in allocating other economic goods.

The economic aspects of some other regulatory problems are well enough recognized, but we seem to have become tired and discouraged. The role of regulatory commissions has increased and will continue to increase. The import of their decisions on the economy is tremendous. To help them to understand their jobs better and to develop for them an improved rationale of procedure is a major responsibility of economics.

The third traditional area consists of monetary-fiscal-employment-inflation problems. I do not question that great progress has been made here during the past thirty years, in the understanding of the problem by economists and in public education and policies. But I fear that the backlog of demand accumulated during World War II, the heavy military expenditures of the '50's, and some good luck have made us too complacent. I for one would be amazed if we did not have some very troublesome depressions in the '60's, and I am far from convinced that we have the knowledge, institutions, or will to cope with them promptly and adequately. Even if my relative pessimism is unjustified, this is an area that bristles with thorny secondary problems. Short-run economic predicting is in its infancy. The appropriate criterion for compromising the claims of full employment and those of price stability remains elusive, as does the criterion for selecting suitable blends of monetary and fiscal measures.

The fourth traditional area in my classification is taxation and all that—the revenue side of public finance. The Ford

Foundation's handsome grant to Brookings ensures that it will not be neglected here. And it is time for a fresh, comprehensive look. Circumstances have changed since the classics of public finance were written. Our tax structure is inherited from periods of low peacetime expenditure and a wartime emergency; it would be a remarkable coincidence if it were appropriate for a period of sustained high government expenditure.

* * *

Let me turn now to the less traditional areas, of which I should like to discuss two: the economics of development and growth, and the economics of government expenditure. Neither, of course, is wholly new. The early classical economists were much interested in growth, and most works on public finance have included a reference, or in some cases a short chapter, on the principles—usually pretty empty principles—that should govern expenditure. But growth was neglected for a long period, and the study of public finance has been steeply slanted toward the revenue side.

In the case of development and growth this neglect has been redressed with a vengeance in recent years for the underdeveloped areas. Two years ago an economist friend remarked that he and I were the only economists of his acquaintance who had not developed an underdeveloped country. Since he has spent the last year developing one, I feel very much alone. I do not quarrel with this emphasis. I know no set of problems as important in shaping the future course of the world.

But developed countries have their growth problems too. Those of us who have difficulty recognizing the affluent society cannot help wondering, especially in an election year, where the resources are coming from to accomplish all the good things desired by both party platforms and American society—including, of course, aid for the development of the underdeveloped world. Short of the miracle of peace and disarmament, most of the needed resources must be made available through growth.

And hopefully some exciting new research is beginning to increase our understanding of the growth process, of the neglected factors on which it depends, and of the role of public policy in promoting growth. Briefly, it has revealed that by far the larger part of the increase in productivity in western economies is accounted for by the baby we used to throw out with the bath water when "for the sake of simplicity" we assumed a constant state of the technological art. We have moved far from the age I can remember when a Cobb-Douglas function was taken seriously as an explanation of the growth of output over periods measured in decades. The essence of growth, in the civilian economy as with our military capability, is research and development, invention and innovation. This is not the only element in growth, but it is tremendously more important than investment within a given state of technology, and the attention we have devoted to these two factors in the past has been in inverse proportion to their importance.

What public policies are conducive to growth? In the past, economists confronted with such a question would have started enquiring about the state of saving and investment. Was it

adequate? How might it be stimulated by monetary or fiscal measures? Let me suggest that there are more important questions to ask—at least in developed societies like our own. First, are we devoting enough resources to research and development, to invention and innovation? There are very persuasive reasons for believing that a competitive *laissez-faire* economy does not do so; that this is the realm in which the gap between private and social products is broadest and deepest and most damaging to growth. And second, if the answer is negative, how can we stimulate more, and how do we do so in the most efficient and productive manner? Was Schumpeter right in arguing that we should temper our antimonopoly policies in the interests of innovation? Is there a promising role for the trade association or the industrywide R & D center? Are there desirable tax inducements? (Here I think the high rates of corporation income tax, whatever their other adverse effects, have yielded a windfall by effectively subsidizing half the cost of corporation-financed research and development.) To what extent should government finance or subsidize industrial research in general, or for certain industries, as it has already done so productively for agriculture and defense? And how, when government is paying the bill, can it rationally choose areas and projects and minimize the dead hand of bureaucratic control and "cost plus"? Are our patent laws the best means of stimulating either the private or the corporate inventor?

None of these questions is, strictly speaking, new. But a substantial shift in emphasis is demanded. In the past they have been the domain of a few specialists; they belong at the

core of the economic theory of growth and development and they should be at the forefront of our thinking about public policy. Brookings has begun to enquire into government contracting policies in the R & D area and has started to think about the economics of space development. I predict and hope that it will move rapidly into the central issues. My own thinking here, as elsewhere, has been colored by my work on the economics of defense. The tremendous growth in military capabilities—our own and the Russians'—over the past twenty years has been the consequence of research and development—deliberately planned and lavishly financed. Lavishly, that is, by contrast with earlier periods and with any private industry, although R & D expenditures have been a small fraction of total military expenditures. But these relatively small expenditures have been staggeringly, alarmingly, productive. Without the new military technologies created by R & D, no amount of military investment—not even investment of a magnitude to be measured in GNPs—could have produced a remotely equivalent growth of military capabilities. Even if we ignore the single overwhelmingly important development—that of nuclear weapons—this conclusion is modified only in degree. I have tried to think of reasons why the military area should be unique in this respect: I can think of none. Our somewhat similar experience in agriculture, on a much smaller scale, suggests that it is not. I suspect that there has been a serious misallocation of resources, and that it corresponds to economists' misallocation of their effort between problems of investment and problems of technological change.

The other new, or relatively neglected field, is the economics of government expenditure. It is immensely encouraging that the Ford grant to Brookings places research on expenditure on a par with research on taxation. It is not hard to find reasons for the neglect of government expenditure in the past, for until recently government expenditures or production in peacetime have been small—a relatively insignificant part of the economy. It did not much matter how efficiently it operated. Moreover, economists found it difficult and uncongenial. Our theory was tied to prices and markets, and in the government sector the market structure was either totally lacking or incomplete. Of course, times have changed. In every western country government expenditures are pressing or exceeding Colin Clark's magic 25 per cent, and while the public 25 per cent is less than the private 75 per cent, if we take into account our past neglect of the economics of government expenditure and consider the potentialities for improvement, the public sector begins to assume for the economist the same order-of-magnitude importance as the private sector—i.e., that part of the private sector not working on government contracts. If we don't have an adequate theory of efficiency in a nonmarket economy (and we certainly don't), it is high time we developed one.

For the problems of government expenditure are economic problems—i.e., they are economizing problems. And the first helpful thing we can do as economists is to recognize them as such and persuade noneconomists that they are economic problems. As I stressed much earlier, even this step can be useful. The mere arraying of alternatives, and straight thinking about

their relative costs and utilities, can sometimes throw a flood of light on expenditure decisions. And although a good deal of progress in this direction has been made during the past ten years, particularly within the military, this mode of thinking is still uncongenial to many in the Budget Bureau, in the spending departments, and in Congress. We are still told that we must provide so many antiaircraft and antimissile battalions per city for air defense because that is the military "requirement." And in California we have just approved a \$1.75 billion bond issue to finance the Feather River Project because officials and voters were persuaded, with no hard look at costs, alternative sources of supply, or possible reallocations, that we will "need" so many million additional acre-feet of water—mainly, it would appear on analysis, to subsidize the irrigation of alfalfa and surplus cotton.

As this experience indicates, the mere recognition that government expenditure problems are economic in character is not enough. We will usually need an economic analysis which measures the costs and utilities of the alternatives—or as many of them as can be measured—and evaluates them to determine which yields the greatest (or a greater, or a great) excess of utilities over costs.

I am far from suggesting that this kind of analysis is easy or that all economists will like to do it. It is hard—partly because it is messy and empirical and requires knowledge outside the field of economics, and partly because government objectives are plural and frequently incommensurable, and those handy economist's crutches, market prices, are unavailable or

incomplete. Nevertheless, we know now from experience in the military sector, in natural-resource development, and in transportation and utilities, that good, useful economic analyses of this kind—partial analyses—are possible.

The economics of government expenditure is rather closely analogous to management economics, where there have also been highly significant developments during the past decade. In management economics there is the same difficulty of plural, incommensurable and ill-defined objectives, similar problems of spillover effects and "game" elements, and an incomplete structure of market prices (within the firm). But I find the problems of government expenditure more challenging. The difficulties, while similar in character, are greater. And since public policy is involved, success seems more rewarding.

The key to progress, I am convinced from my own experience, is modesty in our objectives. It is perfectly apparent that when we have incommensurable and ill-defined objectives, incomplete markets, and substantial uncertainties and game elements, the *optimum optimorum* will be elusive. Fortunately, to be useful, economists do not need to find that Holy Grail. The practically important conclusion is that A is better than B, or even that A is better than B *if*. . . . And that is much easier. If economists can learn to do that much well, we can begin to make some sense of expenditure decisions.

Finally, in the government-expenditure area, economists can be useful by suggesting improvements in the institutional environment. Our traditional budgetary practices seem designed to defy economic rationality. The patterns of incentives for

officials at all levels almost guarantee that parochial criteria will dominate national ones—divergences between private and social costs and benefits are as pervasive in the public sector as in the private. A large proportion of the energies of the best people in government are consumed in fighting for larger budgets rather than in making the best use of those they have. Our departments and bureaus have functions assigned in such a manner that alternatives which could be the objects of rational choice within one become bones of contention among several. Is this arrangement—of which interservice rivalry is the most notorious example—as bad as it appears at first sight? Or is it, as Ed Lindblom has suggested, an imprecise but important and improvable means for ensuring that objectives broader than those of the single bureau are given due weight in the decision-making process? Economists, with all their predilection for improving the institutional environment, have left government organization, one of its most important elements, almost exclusively to the public administration profession. I think that economics contains some insights that could be useful in improving government organization—insights into interrelated decisionmaking at different levels, into incentives and the rules governing the necessity and sufficiency of information for decisions. At least I hope that some economists will feel challenged to give it a try, and I can think of no more appropriate place for the attempt to be made than Brookings.

I promised to say something in conclusion about the usefulness of that strange breed—the economist. I think I have said quite enough as I have gone along on the positive side. Econo-

mists have a training and a mode of thinking which makes them useful on many public policy problems, even surprisingly useful—useful on problems that noneconomists and many economists do not usually regard as economic.

But we also have some negatively useful characteristics in tackling such problems, a considerable dose of what Veblen so aptly termed "trained incapacity." First, and most frustrating, is the passion of the economic theorist for unequivocality and perfection. The typical welfare theorist, for example, wants to wash his hands of a problem if he cannot prove, with certainty, that the lot of every single individual in a society will be either bettered or not worsened. Let me suggest that a part of his intellectual difficulty may stem from his tradition of considering each decision in isolation. I think that *distributive* effects and *spillover* effects, as well as uncertainties, become much less inhibiting if we think instead, and much more realistically, of the appropriate criteria for a large set of decisions. Consider two economies, one of which consistently adopts changes which seem likely to increase efficiency in particular industries, sectors, departments, etc., without worrying too much about scoring 100 per cent on each and every decision, or about remote distributive and spillover effects; while the second never makes such changes—or leaves them to chance or to other influences—because the outcome is uncertain, or there may be negative spillovers which can't be predicted or measured, or because some individual may, as a result of this particular decision, be worse off. Is there any question whatever which of these economies will grow faster, which will be the

stronger and more productive after only a few years? I am far from arguing that uncertainties, distributive effects, and spillovers can be neglected. I am arguing that perfectionism can stultify otherwise good economics. And I am arguing that in developing appropriate criteria for numbers of decisions (as contrasted with isolated, critically important decisions) there is much to be said for relying mainly on "expected value" calculations, ignoring much of the variance, and for concentrating, in the case of remote spillover and distributive effects, on expected *bias*. If there is good reason to suspect that all or most of the decisions in some area will have predominantly unfavorable spillovers, or that all will, for example, sock the farmer, then let us by all means study them. But let us not inhibit decision and action because of the mere probability or even the certainty that there will be some negative as well as positive spillovers from particular decisions. The classical economists who argued for freer trade were rightly not so inhibited.

Another trained incapacity stems from our past emphasis on static models. We became so bemused by static theory that we usually forgot that profit maximization is an ambiguous or multidimensional criterion. I have already discussed the implications of this traditional mode of thinking for the study of development and growth. Uncertainty is the essence of R & D, and we will not get far with its analysis and with appropriate public policies for promoting it without a quite sophisticated understanding of this essential property. Fortunately, enough economists have had enough training in statistical theory that we score better than most professions in this respect. The litera-

ture of game theory has also broadened our horizons somewhat. But we have far to go.

Thirdly, we have fallen into the lazy habit of making convenient but naive assumptions about some of the critical inputs of economic analysis—notably naive assumptions about individual, business, and government behavior. I will say nothing here about our assumptions regarding individuals and businesses, about which a considerable literature has been developing. But our frequent naive assumption that the government is a monolithic entity devoted only to the public welfare and knowledgeable about how to attain it has had several most unfortunate consequences. As I have already pointed out, it has closed some promising fields to economic analysis, e.g., government expenditure and government organization. It has caused us to neglect the all-important problems of acceptability and implementation in making recommendations for public policy. And it has even led some of us to recommend government action on the quite insufficient grounds that a function is performed less than perfectly in the private sector. As George Stigler loves to remind us, this is like the Emperor, judging two singers, who awarded the prize to the second as soon as he heard the first.

And finally, like the practitioners of every science, we have a marked tendency to go it alone. Every economist who comes to RAND is required first to unlearn the phrase "as an economist"—"As an economist, there is nothing I can say about that problem," or "As an economist, all that I can say is. . . ." Our new problems and the residue of our old problems are not very

tractable to the skills of the economist alone. Promoting economic growth is an economic problem, but much more than an economic problem. Choosing good transportation systems is an economic problem, but it can't be solved without sociologists and engineers. Allocating frequencies is an economic problem, but the economist who tackles it without knowing a good deal of electronic technology, or getting help from someone who does, is asking for trouble and frustration. And those problems of acceptability and implementation, which we can't ignore without being irresponsible, require sophistication in politics and psychology. Perhaps some of our more traditional research areas, even in the public policy field, were more purely economic. But in the future, as I see the future, economists, if they are to remain useful, will have to tackle a lot of impure problems for which their special tools, while useful, are not enough.

Education is an integral part of the Brookings program. Perhaps it should be extended somewhat to improve education *of* economists, as well as of noneconomists *by* economists.